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The Institute of Development Studies - at the University of Sussex - Falmer - Brighton - BN1 9RE

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Explaining the Violence Pattern of the Algerian Civil War

Roman Hagelstein*

roman.hagelstein@uni-tuebingen.de

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Abstract: I draw a geographically and temporally disaggregated model of the location and course of the Algerian civil war, using new battle event and location data from press reports. I show that the war was located in areas and at moments in time in which both the rebels and the government were about equally strong, according to my novel relative strength index. Additional factors that can robustly predict high location-specific war intensity are the severity of violence at a location in the past period, and unemployment. Finally, violence is unlikely to take place in unpopulated areas.

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* Universität Tübingen

Contents

Abbreviations

ACLED Armed Conflict Location and Event Dataset
AIS Islamic Salvation Army (Armee Islamique du Salut)
ANP National Popular Army (Armee Nationale Populaire)
CSCW Center for Studies of Civil War
CIESIN Center for International Earth Science Information Network
DZD Algerian Dinar
FIS Islamic Salvation Front (Front Islamique du Salut)
FLN National Liberation Front (Front de Liberation Nationale)
GIA Armed Islamic Group (Groupe Islamique Arme)
GSPC Salafist Group for Preaching & Combat (Groupe Salafiste pour la Predication et le Combat)
HCE High Council of State (Haut Conseil d'Etat)
IMF International Monetary Fund
JORA Official Journal of the Algerian Republic (Journal Officiel de la Republique d'Algerie)
MIA Armed Islamic Movement (Mouvement Islamique Arme)
NGA National Geospatial Intelligence Agency
RGSI Relative Government Strength Index
RRSI Relative Rebel Strength Index
SEC Strength Equality Condition

1 The Algerian Civil War

The Algerian civil war has been lasting from 1992 to 2002 and lingers on on a low level since then. It was sparked by the interruption of the first Algerian legislative elections, which the Front Islamique du Salut (Islamic Salvation Front FIS) party was about to win. On January 11, 1992, the Army deposed President Chadli Bendjedid in a bloodless coup, outlawed the FIS, and sent most of the FIS' senior members to detainment camps in the Sahara. The Army then took power, establishing the five man junta "Haut Comite d'Etat" (HCE) as the executive body for Algeria.

In the case of Algeria, high vulnerability to civil war onset due to the mentioned change in polity (Collier and Hoeffler 2004), that went along with democratization in 1989-1991, was exacerbated by low hydrocarbon prices. These led to a virtually bankrupt government (Martinez 2000:92) and blatantly exposed the regime's problem of widespread corruption (Auty 2003). This weakness of the incumbents was exploited by the FIS oppositon electoral program, which could be called populist by most standards (Roberts 2003:162)[1]. When the FIS was outlawed, various islamist guerilla groups soon began to take up arms and combat the government.

This paper's aim is not to explain war onset, which has been analyzed in detail already in Lowi (2006). I also want to go beyond a simple descriptive analysis of the course of the Algerian civil war, for which an excellent source would be Martinez (2000). I will, in contrast, build a geographically and temporally disaggregated model of civil war violence to predict and explain the location of war events and intensity and duration of the war in Algeria.

This paper is organized as follows: section 2 presents the theoretical framework with which I will operate, section 3 derives several hypotheses on the basis of this theory; section 4 presents the empirical analysis, and section 5 concludes.

2 Theory - Why Study the Algerian Civil War's Violence Patterns?

My objective is to develop a model that determines location and timing of civil war events in Algeria. In order to achieve this goal, I will build on resurgent research about relative strength of civil war actors at a location (see e.g. Buhaug 2007, Kalyvas 2006, chapter 9). The aim of this paper is to predict which local constellations lead to tragically high levels of violence. This is important for several reasons. First, a high level of civil war violence kills and maims people. In addition, it stifles economic growth. Collier et al. (2003), for example, argue that each passing civil war year throws a country further off its prewar economic growth path.

In addition, thorough analysis of the Algerian Civil war as one of the recent civil wars in

[1] Among other aspects, the FIS had "virtually nothing to say about economic policy" (Roberts 2003:83)

the Middle East may provide an additional understanding of typical civil war patterns in the MENA region, which has seen a recent upsurge in civil warfare, be it in in Iraq, Lebanon, Afghanistan or Sudan.

Thus, the ability to locally determine which institutional mix leads to higher amounts of violence, and which one decreases violence, is crucial. There is considerable macro evidence towards the statement that violence arrests development (e.g. Collier 2007). This evidence probably also holds for village-level analysis, the de-facto subject of this study. One town or area affected by violence more than its neighbors apparently suffers from higher trade barriers as a result. Roadblocks, for example, were very common in Algeria and manifestly affected trade. Furthermore, the shattering of confidence and trust may severely impede productivity-enhancing relationships.

Another reason to use geographically and temporally disaggregated battle-data is that this approach offers crucial information concerning civil war duration that cannot be accounted for in cross-country comparisons of war duration such as Fearon (2004).

Similar models of violence within civil wars have only recently become the focus of the research community. Hegre, Ostby and Raleigh (2007), for instance, develop a spatially disaggregated model of civil war violence using Geographical Information Systems. Halvard Buhaug and Jan Ketil Rod (2006) analyze "Local Determinants of African Civil Wars", and Hegre and Raleigh (2007) also perform a geographically disaggregated analysis of African civil wars. In principle, this literature's objective is to avoid the ecological fallacy inherent to highly aggregated country-level data, which is typical in the study of civil wars (Rod and Buhaug 2007). Aggregated country-level data, while providing a good overview of conflict onset, does not provide researchers with a sufficient understanding of the dynamics (duration and termination) of violent conflict. For instance, wealthier countries are less prone to civil wars (Fearon and Laitin 2003, Collier and Hoeffler 2004). Yet, this paper argues that within the countries experiencing conflict, richer areas may be more prone to violence.

Indeed, high "target value" may induce the conflict's actors to fight harder, because their interest to tax these areas is higher. This is the base on which Hegre, Ostby and Raleigh (2007) ground the inclusion of wealth as explanatory variable in explaining civil war violence levels. "Both parties to a conflict will target strategic locations such as crossroads, bridges, ports and airports held by the opponent, and invest resources to protect them. Another factor is the extent to which the location can provide revenue to the parties." (page 5).

I argue that the empirical observation that higher wealth leads to bloodier conflict intensity can be mainly traced back to Hegre, Ostby and Raleigh's secondary factor, *taxing opportunities*. Both actors in a civil war, incumbents and insurgents, must decide where to allocate their scarce resources. Each actor has an interest in securing sources of funding, as a civil war is much more costly than normal political competition. Evidence in support of this argument in the Algerian case is the government's extreme dedication of resources towards securing the oil wells and pipelines during the war (Le Monde, May 7, 1995). The rebels, in turn, needed to find other sources of financing.

Martinez (2000:107) argues that a large array of economic incentives for small-scale war-

lords in the Mitidja plain, the area south of the capital, arose from the conflict. Such warlords "protected" any merchandise traffic from and to Algiers. Martinez states that "the permanent establishment of armed bands in areas like Cherarba, Baraki and Les Eucalyptus[2] is explained not only by extreme ardour for waging the Jihad, but also by the abundance of financial resources due to the high proportion of petty traders whose business was expanding." He adds that the "main roads filled with vehicles favoured protection rackets and explained the proliferation of fake checkpoints.". Thus, both actors of the civil war were in constant competition over the country's economic resources. This competition was reflected in higher levels of violence in wealthier areas.

One of the main further contributions of this paper is the formal inclusion of an index of relative strength. It is inspired by the works of Buhaug (2007) and Kalyvas (2006) who both developed similar models. Stathis Kalyvas discerns violence levels for five zones of control. He argues that:

- "Where levels of control are high, there is no defection, no denunciation, and no violence." (p. 203)
- "Under fragmented control, violence will be exercised primarily by the political actor enjoying an advantage in terms of control"
- "Parity of control between two actors is likely to produce no selective violence by any of the actors". (p. 204)

Thus, geographic areas in which one actor is marginally stronger than the other actor will witness most events of violence, according to Kalyvas' model. This last idea is "in contrast to Arendt's (1970:56) implication that the highest level of contestation should breed the most violence because it is precisely where power is in jeopardy" (p. 204). A novel index of relative strength allows me to test empirically which idea offers better explanatory power for the Algerian case.

In addition to testing the two main hypotheses mentioned above, I add a set of control variables to the model. First, I control for war-related violence levels in the past years. Hegre, Ostby and Raleigh (2007:23) note that "events are not independent - an attack by one actor in a location is likely to lead to repeated attempts if unsuccessful, and to retaliations by the other actor if successful". In addition, repeated evidence in Sidhoum 2002-2006 leads me to suspect that especially government militias were difficult to keep under control after insurgent assaults, especially in their retaliation against actual or presumed rebels. Finally, this accounts for unobserved location-specific factors that make the location more prone to conflict.

In addition, *population density* can be a powerful instrument in predicting civil war events. A higher population density eases urban insurgency tactics, and minimizes casualties for insurgents. Attackers can hit, run and merge with the local population. Moreover, if one

[2]all southern suburbs of Algiers

assumes a constant per-capita propensity to fight, more populous areas have a larger number of recruits to draw from (Hegre & Raleigh 2005). Finally, I am counting events of violence against humans; it is more difficult to hit someone where no one lives. The argument that high population density is part of the technology of insurgency is not entirely new. Mao Tse-Tung (1948) famously argued that "The people are like water and the army is like fish" which must swim in this "water" in order to survive.

Together with partial dismantling of the inefficient state enterprises, large amounts of unemployment were produced. Unemployment, in turn, can be seen as reducing the *opportunity cost of violence* and lead to conflict, as theorized in Collier and Hoeffler (2004). Unemployed people may be more willing to incur risky behavior for making a living. I consequently model it as another factor making violent conflict worse.

In sum, this paper seeks to explain location and timing of violence in the context of a civil war as a function of an array of factors. These are target value, control levels, population density, polarization, opportunity cost, and external intervention.

3 Explaining the Violence

3.1 The Dependent Variable: Location of the Events

For the purpose of this study, I coded a spatially and temporally disaggregated event variable in the Algerian civil war from news reports. It provides information about the geographic location of the battles covered in the media. In addition to reported clashes between the security forces and the insurgents, I systematically included one-sided violence when it was reported. About three quarters of the reported violent events were one-sided events, and a large number of these were bombs, targeted assassinations and massacres of civilians. They form an important aspect of the logic of the war. Often described as 'wanton' and 'senseless' (Kalyvas 1998:1) they can in fact also be explained empirically as part of the war strategy of both combatants. They serve various purposes. Massacres, for instance, secure allegiance by example. They indirectly threaten the surviving civilian population that the same will happen to them if they do not comply with the perpetrator's desires. The purpose of bombs and targeted assassinations - also one-sided events - is more straightforward. A weaker conflict party that would probably lose out in a pitched battle with its opponent will use hit-and-run tactics as the least costly method of inflicting damage to the stronger party.

Figure 2 shows the location of all[3] events from 1992 to the end of major hostilities in 2005, in the center of the country. Stars represent one-sided events, and triangles show battles involving both the security forces and the insurgents. Dark lines represent roads (from ESRI

[3]Unique events drawn from at least one of my four sources detailed below, which featured information about the location and time of the event, for which the probability of being criminal violence was low, and excluding one-sided events related to the kabyle riots of 2001 and 2002.

2006)[4] Lighter shades indicate a low election result for the FIS (below 35%), while darker shades indicate a high score (more than 50%). The area with most events on the map is the Algiers-Boumerdes urban area in the top-middle.

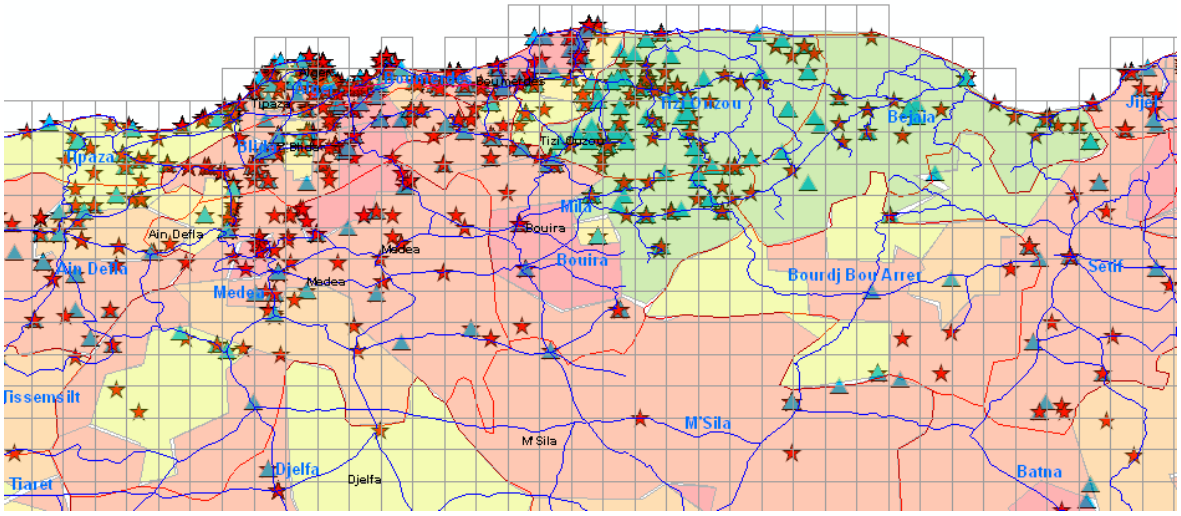

Figure 2: Civil War Events 1992-2005 in the central Algerian provinces.

For determining location and time of the 3813 codeable one-sided and 1685 codeable two-sided events in the war, I could rely on several sources. My main source, however, is the chronology of massacres in Algeria (Sidhoum 2002-2006), which I completed with a large number of newspaper reports[5]. Sidhoum's chronology is the only reliable and complete source by 1997. Before 1997, I could find reports of violent events in Le Monde which it did not cover, later on this was not possible. Its own sources include newspapers, both in French and Arabic, and eyewitness accounts. Newspapers were heavily censored from 1994 to 1998, and the eyewitness accounts are thus mostly from Sidhoum and his collaborators, who lived in Algiers at that time.

I coded a battle if a clash or an ambush between one of the two rough parties of the conflict, the insurgent groups and the government, occurred, even if no figure of the amount of people killed was available, or none were killed. I also coded as battle any reported search operation by the Army, or "ratissage". One-sided events include among others bombs, roadblocks, and war-related killings. Both need not be lethal, but they have to be war-related. For instance, if a bomb was defused, causing no casualties, it is coded as a one-sided

[4]The color overlay shows the election results of the 1991 legislatives for the FIS by electoral constituency, a part of the relative strength and polarization indices (see below) which I recoded from Fontaine (1992)'s map.

[5]Le Monde 1992-1998, BBC Online 1998-2004, and El Watan Online 2004-2005. Le Monde unfortunately had no correspondent in Algeria from 1997 onwards.

event.

Coding events, several difficulties arose. First, disappearances could not be coded since press reports about them were suppressed. Second, there is a major problem with one-sided events. It was at times difficult to determine whether the event was criminal violence or war related, especially in the case of targeted assassinations. One event, for example, got ample media coverage - the case of Katia Bengana. This 17-year old high school girl was killed, supposedly for having refused to wear the veil. In fact, this case was likely criminal violence unrelated to the conflict[6]. It is thus not coded. Actor data in the database (present, but not used in the analysis for lack of reliability) is highly unreliable, especially when faced with one-sided violence - the incentive to blame the opponent for one's own atrocities is high for both sides[7]. I coded it best as I could.

I have coded any event from 1992 to December 31, 2005 even though the conflict has been classified as "minor" by PRIO/CSCW (2006) since 2003 for failing to meet the "war" death toll of 1,000 people per year. The official death toll is 150,000[8] to which one must add massive internal displacement. If one accepts the President's statement, out of a population (2006) of just under 33 million[9] (ONS 2007), 0.36% was killed during the course of the conflict. The most violent years, 1997-98, may have seen up to 60 dead per 100,000 population, a war twice as violent per-capita as the Iraq conflict as of 2004, or about as deadly as the war in Sierra Leone at its height (Human Security Report 2005). A discussion of these discrepancies in casualty figures would constitute a highly interesting complement to this study. But without access to original government data sources, it would also be annoyingly speculative.

3.2 Economic Explanatory Variables: Target Value, Population, and Resources

I use the proportion of automobiles per person at the province level from the official statistics (ONS 1996-2005) as an indicator of wealth and thus target value at a certain location.

[6]"28 fevrier 1994: Une lyceenne de 17 ans, Katia Bengana est tuee par balle Meftah pour avoir refuse de porter le hidjab selon la version officielle. Attentat amplement mediatise en Algrie et en France. Quelques mois plus tard, et lors d'un meeting des eradicateurs outre Mediterranee, une oratrice se disant etre l'amie de la defunte revelera qu'il s'agissait d'un drame passionnel n'ayant rien voir avec les islamistes et le hidjab. Stupeur dans la salle." (Algeria-Watch 2002)

[7]See e.g. Yous (2000) in particular concerning the large massacres of 1997 and 1998. Yous argues that the Benthala massacre was the work of a secret service proxy posing as an Islamist organization. There is still some debate concerning the identity of the perpetrators.

[8]Bouteflika 2005: Speech on February 25, but this estimate is probably inflated. My database codes about 26,000 killed, to which one must add 18,000 disappeared for a lower bound of total casualties. Indirect casualties were probably not overwhelmingly high since the database also uses all available information on lethal one-sided war-related violence. In addition, the President may have an incentive to overstate the extent of the conflict for outward and inward legitimacy. Access to his data source would of course be very enlightening.

[9]Population growth was very large in the conflict period: in 1992, the Algerian population numbered about 25 million.

This indicator is unavailable for 2002 and before 1996. I extrapolate it back by using the amount of cars in 1996 as a proxy for the missing data before 1996, and the 2003 number for 2002. The ratio in question varies strongly between provinces, but does not increase or decrease significantly between the years[10]. I use that indicator as a necessary surrogate to the provincial level of GDP per capita, which is unavailable. A major advantage of automobile registration count as a wealth indicator is that this indicator is difficult to manipulate, and as such likely a very objective indicator of accumulated real provincial wealth.

H1: The higher the ratio of cars per capita at a location, the higher its target value. Competition for taxation of these areas is higher.

3.3 Government and Rebel Strength

In this section, I will build an index with which to proxy strength equality. Recognizing that this paper needs to find a proxy for locations in space and time in which the strength of the civil war's actors is similar, I have identified an array of factors increasing government strength, and factors increasing rebel strength. I will subtract rebel strength at a certain location from government strength at the same location. Buhaug (2007) states that state strength at a location and a point in time depends on "a combination of allegiance and coercion". Thus, state strength is proxied as follows.

$$SEC_t^j = |\text{Government Strength}_t^j - \text{Rebel Strength}_t^j| \quad (1)$$

With indexes j and t denoting the location and time of the observation. Rebel strength is subtracted from incumbent (government) strength. If this SEC index is negative, the government is weaker than the insurgents at a certain location and in a certain year. If it is close to zero, both parties' strength is about equal. If it is superior to zero, the government is stronger. In my analysis, I use the absolute value of this index as shown in equation 1 above - my thesis is that strength equality of both parties increases the probability of battle events taking place at a certain location and a certain time.

According to Buhaug (2007), the geographic diffusion of both government and rebel power decreases both incumbent and rebel strength, if we are moving away from the capital and the rebel bases, respectively. Power diffusion, according to Buhaug, is affected by "quality of infrastructure, extent of local administrative bodies, rough terrain, and cultural differences".

Factors increasing government strength. My RGSI (relative government strength index) is a government strength index relative to initial government strength, the latter measured as government budget in the reference year 1993. I use 1993 as the reference year, and as the first year of analysis, since the IMF (1998, 2003, 2007) data, which is crucial for my analysis, is only available from 1993 onwards. The year lost to the analysis, 1992, included 53 events[11]. In addition, using 1993 as the reference can be justified by some evidence stating

[10] In fact, data seems to be collected every two years only, even if it is reported for every year except 2002.

[11] or 0.25% of total events, of which 46 one-sided

that the rebels showed any organizational structure only as late as winter 1992-93 (Martinez 2000:198), one year after the 1992 crackdown on FIS members.

Relative Government Strength is straightforward and given by:

$$RGSI_t = \frac{\text{Government Revenue}_t}{\text{Government Revenue}_{1993}} \tag{2}$$

The RGSI shows a secular increase from its value of 1 in 1993 to 9.6 in 2005, which is essentially driven by increasing hydrocarbon revenue (Table 1).

The distance to the capital[12] diffuses government power. This is a rough proxy, but I can use it to predict the extent of local administrative bodies in the highly centralized state of Algeria. Proximity to a main road (road class 1 and 2 in the CIESIN dataset) decreases the effect of power diffusion. Distance to capital is thus proxied by a decay function of state power away from the capital[13]. The government power at the capital is proxied by the RGSI. The presence of a main road in a square increases government strength by one point.

The formula for government strength at a location and during a given year is thus:

$$\text{Government Strength}_t^j = \frac{RGSI_t}{\text{Distance to Capital} + 1} + \text{Road in square} \tag{3}$$

Factors determining rebel strength. For determining rebel strength in a given year, I develop the RRSI (Relative Rebel Strength Index). This index is more complicated, as I have no directly observable proxy for rebel strength as in the case of the government strength. I assume that the election results of December 1991 reflect the real power distribution, with government (FLN) supporters pitted against rebel (FIS) supporters. This is a very strong assumption, and a dissatisfying proxy for a number of reasons, but I have no better[14].

The FIS got 47.27% of the vote, the FLN 23.38%. The FIS reaped about *twice as many* votes as the FLN (Fontaine 1992, JORA 1-1992). The FIS, thus, is assumed to be about twice as strong as the FLN in my reference year, 1993. This assumption likely makes me overestimate rebel strength.

In order to track the evolution of rebel support over time, I first assume (see election results) that the rebels were twice as strong as the government in 1993. The index takes the value 2 in 1993.

I then track the percentage of two-sided events in the sample in which the rebels attacked, and use it as a proxy for each year's variation in rebel support. I use as base period the year 1993, in which the rebels attacked in 13% of battles. The RRSI is given as follows:

$$RRSI_t = 2 * \frac{\text{Fraction of Battles in which the rebels attacked}_t}{\text{Fraction of battles in which the rebels attacked}_{1993}} \tag{4}$$

[12] In decimal degrees

[13] In order to avoid dividing the RGSI by zero at the capital, provoking a missing observation in this very crucial square, the RGSI is divided by the distance to the capital +1.

[14] A large part of the FIS' clientele were not 'islamists' but protest voters dissatisfied with the FLN, see Martinez (2000).

Using the proportion of battles in which the insurgents attacked as the time-varying element of their strength may prove fallacious. First, one could argue that this proportion is endogenous to government strength: it depends on government strength as well as on rebel strength. A weak government will attack less often in a given year, and the rebels relatively more often. This leads us to overestimate rebel strength in times when the government is weak. Part of the roadmap for future research could be to disaggregate this index of relative strength or add other operationalizations of relative strength, as in Cunningham, Gleditsch, and Saleyhan (2005).

More importantly, however, incumbents have an interest to selectively manipulate available information in order to downplay rebel strength, and there are hints that they have done so in Algeria (Sidhoum 2002-2006). Battles that could have harmed "morale" of the Army and the population, because insurgents were victorious, were likely underreported.

Table 1 shows the evolution over time of the RGSI and the RRSI.

Table 1: RGSI and RRSI for 1993-2005

Year	**RGSI**	**RRSI**
1993	1	2
1994	1.36	1.82
1995	1.88	2.01
1996	2.58	2.37
1997	2.89	1.86
1998	2.42	2.07
1999	2.96	2.27
2000	4.93	2.51
2001	4.62	3.18
2002	5.00	2.18
2003	6.08	1.96
2004	6.92	2.04
2005	9.63	1.93

As a proxy of local insurgent support, I add one point to rebel strength if the vote for the FIS at a location (Fontaine 1992, data for each electoral constituency) was above 35%. The vote for the FIS was a protest vote, and urban constituencies voted FIS more extensively[15].

I coded several rebel "bases" established throughout the conflict based on Taheri 1998 and Martinez 2000: The Meftah and the Chrea mountains (both Blida province) are coded as GIA/GSPC[16] strongholds. The areas around Lakhdaria, Zharbar (Bouira), the Ouarsenis mountains (Tissemsilt), Collo (Skikda), and the Chekfa mountains (Jijel) are considered

[15]Fontaine 1992:164. Rural poor areas did usually not vote FIS because of "traditional tribal cleavages".

[16]Armed Islamic Group/Salafist Group for Preaching and Combat - the "urban" guerrillas.

AIS/MIA[17] strongholds. Rebel strength will be highest at the location of these bases[18] and decay away from them, the distance for the decay function measured in decimal degrees again.

The fact that "vast territories with scattered population hamper effective rule by increasing costs and limiting the efficiency of policing" (Buhaug 2007) is certainly true for the forest-covered mountains of Northern Algeria. The argument of Collier and Hoeffler (2004) is further widely accepted: rough terrain provides cover for insurgents and as such an opportunity for rebels. My proxy for rough terrain is the average spot elevation per area. Higher elevation increases rebel strength[19]. Because mean elevation varies from 0 (coastal squares) to 1820 m, using it "as is" would overpower the government (which, recall, has a maximum strength of 9.6 at the capital in 2005). Thus elevation is divided by 960 in order to increase rebel strength between 0 and 2 points.

Rebel strength is thus given by:

$$\text{Rebel Strength}_t^j = \frac{RRSI_t}{\text{Distance to Base} + 1} + \text{Vote for FIS dummy 0-1} + \frac{\text{Elevation in m}}{960} \quad (5)$$

Table 2: Components of the SEC

Factors composing the Government Strength Index	**Factors composing the Rebel Strength Index**
Relative Government Strength Index (RGSI)	Relative Rebel Strength Index (RRSI)
Distance to Capital	Distance to Insurgent base
Road in Square	Average Elevation
	Vote for FIS

Coding of the Strength Equality Index is a highly arbitrary procedure. Its arbitrariness is however inevitable, and its rate of change is to be driven primarily by one factor: the secular increase in government revenue. It is deliberately rigged so that in the first year, 1993, the rebels gain power superiority nearly everywhere except in the capital - recall the 2:1 parity justified by the electoral outcome. Thus, in 1993 the strength difference is smallest near Algiers, and as government strength increases secularly over time, the area in which

[17]Islamic Salvation Army/Armed Islamic Movement, the "rural" guerrillas, closer to the ideology of the FIS.

[18]at best the level of the RRSI in the given year, 2 in 1993, plus the one "bonus point" for a local FIS electoral victory, plus at most two bonus points for very high elevation.

[19]An indicator that shows that this assumption is viable is that the government repeatedly bought helicopters and night-vision equipment in order to overcome its disadvantage in poor terrain.

strength parity is observed moves away from the capital. The following table shows how fast. From 1994 to 2005, relative government strength increases, at first slowly, and after 2000 very quickly. In 1998 and 2001, the rebels gain the upper hand in several units of analysis (cell-years, see section 4.4.), essentially because government revenue declined in those years.

Table 3: Cell flips to the government and the rebels

Year	Government to Rebels	Rebels to Government
1993	5726	0
1994	0	27
1995	0	19
1996	0	23
1997	0	133
1998	113	0
1999	0	45
2000	0	827
2001	690	0
2002	0	913
2003	0	887
2004	0	541
2005	0	1648

I use this indicator of relative strength for testing my second hypothesis:
H2: Events will occur where government strength approximately equals rebel strength.

3.4 Persistence of Violence

I proxy population size by the number of people in each of my areas of analysis. The data is a composite indicator of population extrapolated from the four population grids of 1990, 1995, 2000 and 2005 from CIESIN 2007. I log-transformed it because the effect of large population concentrations is unlikely linear, and probably subject to diminishing returns. These grids are likely extrapolated from the 1998 census by CIESIN[20].

H3: With higher population density, the probability of events increases.

The variable that proxies past violence is straightforward: does a higher number of events in the same cell in the previous year lead to more violence in the year immediately following it? In order to retain simplicity and focus on the main hypotheses, I do not include neighboring violence into the model; as will be shown in the results section, this may also not be necessary.

[20]The data were obviously constructed assuming an implied population growth rate of 5% per year in most places. The margin of error may thus be quite high as it ignores significant internal displacement, especially in 1996-99.

H4: Events at a location in the past year likely lead to an increase in the number of events at the same location in the year immediately following it.

3.5 Opportunity Cost of Violence

The government spent its incremental revenue in security and the military at first[21]. After the militias and the counter-insurgency capacity of the military had been built up by the year 2000, the government shifted its focus to welfare[22]. The job market managed to absorb a very large number of unemployed potential dissenters after 2001, the unemployment rate halving from 30% to 15% in merely four years[23]. Combined with an efficient military deterrent, the incentive to join the insurgency for an employed worker is minimal, while to unemployed youth, a war's loot may provide opportunities of social advancement and wealth. My unemployment data stems from the IMF (1998, 2003, 2007) data, originally provided to the IMF by the Algerian government.

H5: Growing unemployment increased social unrest, the reduction of it in the later years of the war brought an end to it.

Absorption of young job-seekers has been orchestrated from 2004 onward with the National Program of Support to Economic Growth, a massive pro-cyclical public works program that pumps more than 1 billion DZD per year (four times the government budget in 1992) into the economy as of 2007, backed by oil money. Such an economic policy is, of course, highly dependent on a high oil rent. It may exacerbate existing rent-seeking (corruption) and inflation problems, and problems brought about by the decreasing labor productivity (IMF 2007b) and high expected wage levels inherent to this statist type of economic policy.

4 Empirics

4.1 Justifying the exclusion of Algeria south of the 32. Parallel

The southern areas of Algeria never played a significant role in the civil war. The only town south of the 32. parallel that was reportedly affected by the war is Bechar, in Southwestern Algeria. In addition to thirteen likely events in Bechar province, evidence about any violence in the South is very shaky. Of the remaining eight events in the database, five were possibly forged by the Algerian intelligence services with the objective to receive United States support and funding (Keenan 2007, Mellah & Rivoire 2005). Of the then remaining three

[21] under the HCE and under Zeroual. Even though President Liamine Zeroual was termed a "dialoguiste" in the press, the military remained a very strong parallel structure to his rule and until a major reshuffle of the structure of the military on February 24, 2000 (Roberts 2003).

[22] One could argue that militia-building is a form of specially targeted "welfare". Groups at risk of joining the insurgency for a livehood, unemployed young males, are selectively bought off by the government.

[23] This is remarkable as the Algerian labor force grew from 6.5 million to more than 10 million in 1993-2005 (IMF 1998, 2007).

events, two were likely criminal violence. The remaining event was an attack of the Army control center in Ouargla, just south of the 32., in September 1992, which would be excluded anyway because the first war year for which I have complete data is 1993.

I thus do not include most of the Sahara in my analysis. Its few outlier events would otherwise, in addition, have a large distorting impact on my results. I am losing 0.1% of the events, 4% of the population, and 74% of the area of Algeria. In addition to eliminating a possible concern about outliers with high leverage and uncertainty that might drive the analysis, this exclusion makes the analysis much more convenient by greatly reducing computing time for the various spatial join operations.

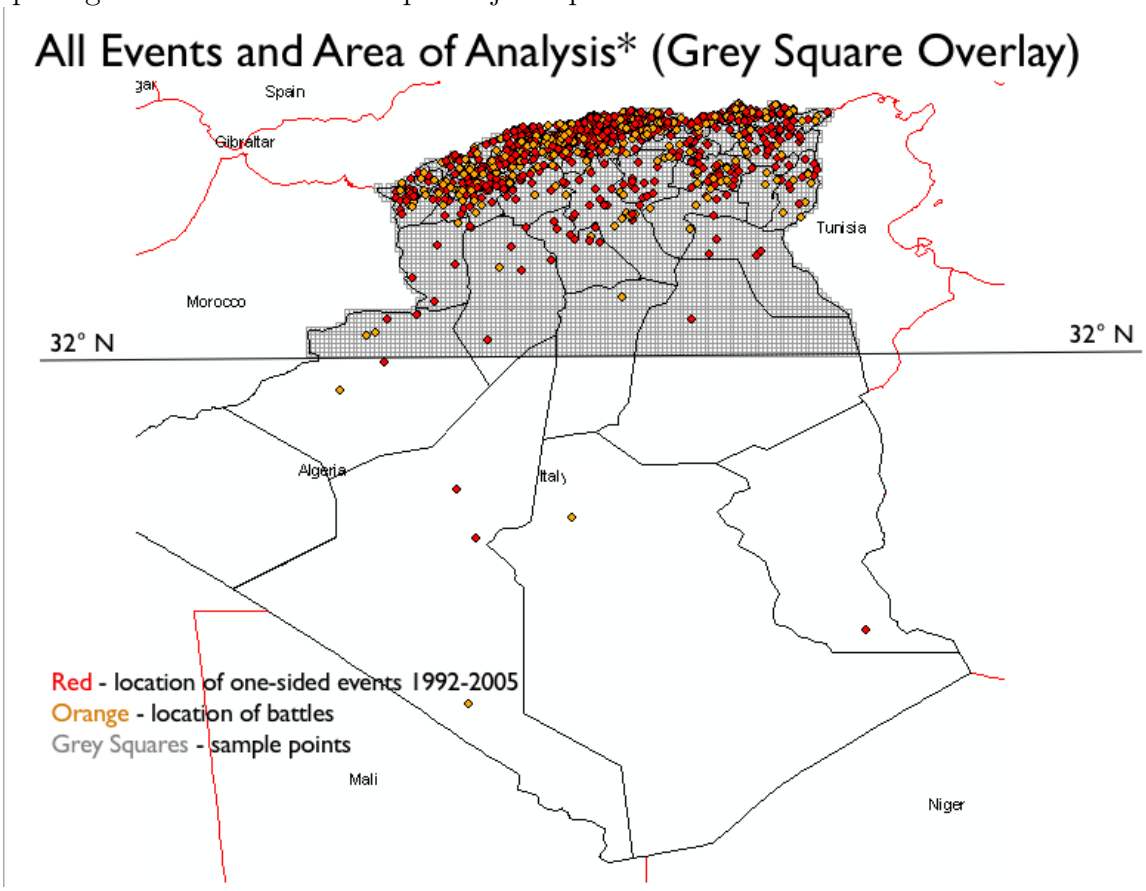

Figure 1: Civil war events 1992-2005 and area of analysis.

4.2 Likely bias in the dependent variable

Roberts (2003:160) refers to 35,000 dead until 1996 in an article written in 1997 under the impression of the estimates of that time. The most frequently used estimation of the death toll up to 1998 is 100,000, first put on the table in a speech from President Abdelaziz Bouteflika in Crans-Montana on June 26, 1999 (Sidhoum et al. 2002). According to these

figures, assuming about 200 dead per month in 1992-93, the death toll must have increased to about 850 per month until the end of 1996 and to about 2,500 per month in 1997-98, and then decreased abruptly to 200 per month in the period 1999-2003 (yellow line in figure 3). Both the 850 and the 2,500 figure are not realistic. They imply a massive increase and subsequent decrease in scale that my event data cannot confirm empirically; my events confirm 17,053 killed only for 1992-98. If I assume that the gap between the recorded events and the official figures is a proxy for the missing battle data, about 120 events per month in 1994-96 (out of 140) and 400 in 1997-98 (out of 450) must have gone unreported.

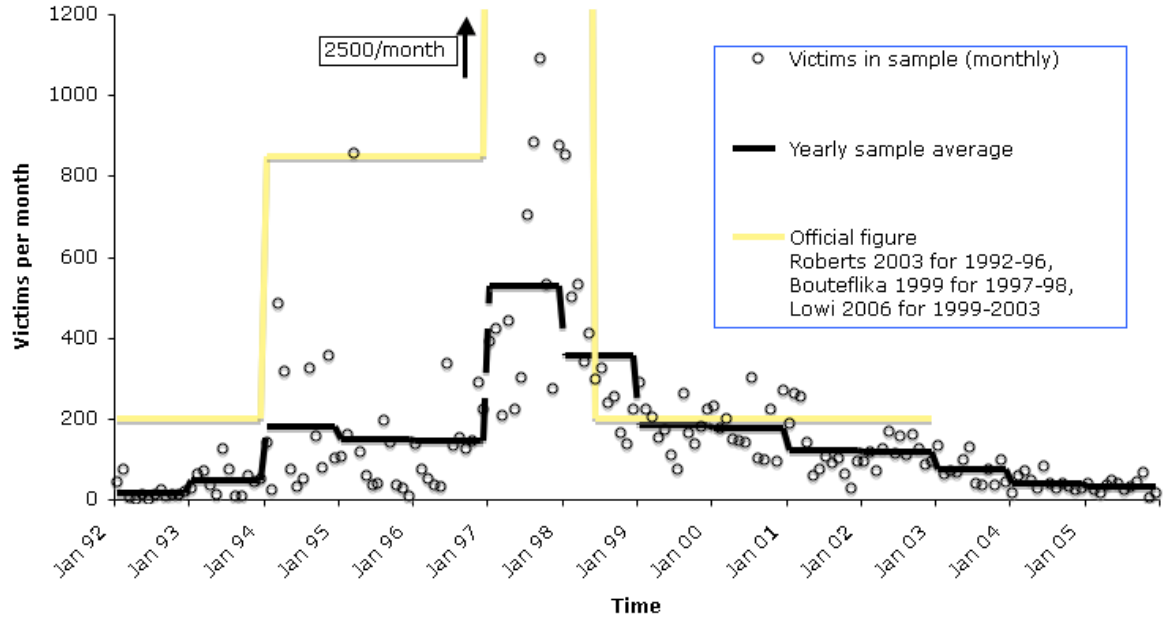

Figure 3: Sample death toll vs. official death toll, monthly.

This point is critical for my work, for if I missed more than 80% of the events prior to 1998, I must expect the sample not to represent the Algerian civil war accurately. However, weighing those events that are more distant to the capital by their distance to the capital left the coefficients unchanged. Thus, even if the data missed a large number of events far away from the capital before 1999, this paper's conclusions remain robust.

I am, however, concerned about the amount of disappearances - one-sided events orchestrated by the government - numbering 18,000 according to human rights activists (El Watan, August 4, 2004), which are not in the data. Including them is part of the road map for future research, though reliable data will probably be a near-impossibility to come by.

4.3 Results

Above are the descriptive statistics for the variables used in the model. Events in a single cell-year peaked at 67, Algiers-Center in 1997. Automobiles per inhabitant, at the province

Table 4: Descriptive Statistics

Variable	No. Obs.	Mean	St. Dev.	Min.	Max.
Total amount of events/cell	81410	.0970151	.9843683	0	67
Automobiles/inhabitant	81410	.0694826	.0264329	.015052	.292085
Difference in strength	72795	.1131908	.082198	4.53e-06	.4434043
Past violence	75595	.1005754	1.013105	0	67
Population (log)	81074	7.135202	1.694096	.751416	14.01595
Unemployment	75595	.2524615	.0420837	.154	.295

level, are between 0.015 and 0.29 (also Algiers province, 2005). The absolute value of strength difference, a constructed index, is explained in section 3.3. Maximum strength difference is 0.44 units. Cell population peaks at 1.2 million in a single cell of 121 square km. It is never zero but below 1000 in 48% of the cells. Unemployment, according to the IMF figures, fluctuated between 15.4 and 29.5 percent of the active population in a given year - it is not geographically disaggregated and a pure time-series. My models analyze several different aspects of the war. The models, unless otherwise stated, use as its dependent variable the total number of events in a given square-year.

A fixed effects[24] zero inflated[25] negative binomial[26] panel regression would be the specification of choice for the above model. Panel fixed effects, tough, drop every cell without any violence from the regression[27]. I thus report both the Poisson panel regression with fixed effects[28] results for those cells in which violence did occur (Model 1) and zero-inflated negative binomial regression results for all cells[29] (Model 2), treating the sample as a cross-section. Population density is used to weed out so-called certain zeros, or cells in which I do not expect any violence at all.

The coarse measurement of cell wealth leads to manifest problems concerning this proxy. In the fixed-effects (within-cell) model 1, what is measured is effectively the rate of growth of cell wealth within each cell. Cell wealth, however, does not increase until 1996 because cell wealth data until 1996 is interpolated using the 1996 data, the first year available. It cannot be meaningfully integrated in a fixed-effects model and must thus be dropped in model 1. In model 2, the cell wealth coefficient is positive and significant, meaning that higher cell wealth indeed leads to higher levels of violence.

The coefficient for the absolute distance to strength equality has the expected sign, is very

[24]The Hausman test is highly significant.

[25]Out of 72795 observations, 97.5 percent witnessed no violence at all.

[26]Negative binomial regression corrects for the fact that events are not independent from each other.

[27]By the way removing the need for zero-inflation

[28]Negative binomial fixed effects regression with more than one independent variable would result in an amount of calculations my statistics software cannot handle.

[29]The Vuong test favors the zero-inflated over the standard negative binomial model. Robust standard errors are used.

Table 5: Model 1: Panel Poisson, Cell Fixed Effects

Variable	**Coefficient**	**(Std. Err.)**
Difference in Strength	-2.110***	(0.211)
Past Violence	0.032***	(0.002)
Population (log)	3.452***	(0.240)
Unemployment	9.061***	(0.431)
N	7913	
LL(model)	-6305.55	
LL(null)	-13155.21	

Table 6: Model 2: Zero-inflated Negative Binomial

Variable	**Coefficient**	**(Std. Err.)**
Dependent Variable: total no. of Events		
Automobiles per inhabitant (interpolated)	9.049***	(1.033)
Difference in Strength	-2.179***	(0.413)
Past Violence	0.543***	(0.045)
Population (log)	0.069	(0.057)
Unemployment	5.152***	(0.840)
Intercept	-4.118***	(0.681)
Certain Zero Prediction Variable		
Population (log)	-1.549***	(0.066)
Intercept	14.084	(0.602)
ln(alpha)		
Intercept	1.697	(0.082)
N	72795	
LL(model)	-9350.85	
LL(null)	-10133.13	

robust across specifications, and is larger than unity. If one moves away from the location in which government and rebels were about equally strong, the likelihood of violence increases more than proportionally. In other words, the lower the strength difference, the higher the likelihood of violence. Hannah Arendt's and Halvard Buhaug's statements, namely that violence is more likely where both actors are about equally strong, can thus be empirically confirmed. For Kalyvas' theory, another model is necessary.

Past violence has a notable influence on present violence, an additional event in the last year increases the likelihood of further violence in the following year. This result, however, is to be interpreted with caution since its impact may be partly driven by omitted variable bias. Omitted and unobserved location-specific independent variables may account for both violence in the past and in the present year. The fixed-effects model corrects for these, and the coefficient is indeed much smaller in this model. Thus, there is likely an omitted variable bias, but also a positive effect of past violence on present violence.

Population is useful in predicting certain zeros, that is, squares in which no events will ever occur - those are the squares with very low population. More than half of the squares have population below 1000. Controlling for this effect (Model 2), larger population is insignificant. High population density does not lead to more violence, but uninhabited areas are unlikely to experience any violence - which by the way strengthens my decision to leave out the Algerian south.

High unemployment in a given year increases the amount of civil war events, as expected. The government was thus right in attempting to buy off potential dissenters in the first years of the war[30].

Disaggregating battles and one-sided events (not shown) leads to reasonably robust results. Thus, there were few significant differences between the drivers of two-sided and one-sided violence. One notable difference is that the coefficient for the unemployment variable is higher if only one-sided violence is explained[31] - a highly interesting empirical result, which tends to confirm Collier and Hoeffler (2004)'s hypothesis. Low opportunity cost of violence leads actors to switch not only to more violence, and more than proportionally to cheaper, less sophisticated forms of violence, if one assumes that two-sided violence is the more sophisticated form of insurgency. The explanatory power of the disaggregated analysis of battles and one-sided events is also lower than the pooled model. In concrete, a random-effects GLS panel model with the same specification as model 2 would explain 98% of geographic and about 20% of temporal variance in violence. A model analyzing only battles explains 78% and 9% of the variance, respectively. Finally, population density is significantly and positively related to violence in all four specifications.

Kalyvas' model (slight strength superiority by one actor induces conflict) performs worse than theory of strength equality. If I replace (or complement, for that matter) the strength

[30]Which in turn may have been bought at the price of a future violence surge. Newspaper reports from 2001 onwards show that many roadblocks were operated by opportunist militia members disguised as "Islamists", especially in Kabylia (Sidhoum 2002-2006).

[31]It is even insignificant for the "two-sided violence only" panel model.

difference variable from model 1 by a dummy that proxies Kalyvas' hypothesis[32], then this dummy variable does not have any significant effect on conflict intensity.

4.4 Caveats

A discussion of cell size. In order to obtain a less arbitrary division of my observation units than provinces would be, I use cell-years as unit of analysis. I sampled the area of Algeria north of the 32. parallel into 5815 grid cells of 0.1x.0.1 decimal degrees, or about 11x11 = 121 square kilometers. This is the grey squared polygon overlay in figure 2. Each cell's attributes are sampled once per year. I have spatially and not temporally varying data (e.g. spot elevation), temporally but not spatially varying data (e.g. the government budget for a given year), and data that is varying both temporally and spatially (e.g. the number of battles in a given square-year, or population density). This method of sampling is the same sampling procedure as used in Hegre, Ostby and Raleigh (2007) on Liberia, except that I added a temporal dimension.

There is no particular reason for using 0.1 decimal degree as cell width. Hegre, Ostby and Raleigh (2007) use 0.08 degrees, failing to justify this cell size. Rod and Buhaug (2007) found out that "what is significant at one spatial scale may not be significant at another", but that this problem is minor. In fact, their cell sizes are five to twenty times larger than mine. Though I have not tested this yet, if I believe Rod and Buhaug's work, results will remain unchanged unless the cell size is significantly increased. The peril of ecological fallacy is addressed in the next section. My justification for using 0.1 decimal degrees is that for such a cell size, most cities and towns - except Algiers, which covers five cells - are covered in one cell, while neighboring cities get their own cell. Some cities, Boufarik and Blida for example, would be aggregated together using larger cell sizes.

Urban Bias. Sampling of battle events is likely biased towards populated places, because of two reasons. Firstly, the accounts often state that an event took place "...close to" a certain town as the most exact geographic location available[33].

Secondly, there is likely a structural urban bias that leads to the chronic underreporting of rural events, as stipulated by Kalyvas (2006:38[34]). I have not thoroughly tackled this urban bias yet. Additionally, the rural dynamics of the war are underreported and poorly understood in the case of Algeria. Violence in rural areas was probably kept under close control of the local authorities, as Martinez (2000:191) remarks, and which would be an

[32] A dummy that equals unity if strength difference is between the twenty-fifth and the seventy-fifth percentile, and zero otherwise, regression not shown.

[33] I have marked cases that did not occur at the exact location at which they are coded in the database according to ACLED (Armed Conflict Location and Event Data, Hegre &Raleigh 2005) rules:
1- the event happened at the exact location
2-in the vicinity of that location
3-in the province (*wilaya*).

[34] "Studies of civil war violence are produced by urban intellectuals despite the fact that most civil conflicts are fought primarily in rural areas by predominantly peasant armies"

Algerian peculiarity. "The murder of a guerrilla or a policeman is the result of a targeted choice, patiently weighed and not supposed to cause any upheaval among the leading people of the village."

The data allows to control for urban bias, in a variety of ways. One way is to estimate a model that weighs rural events more. I have done so already in model 1. Another way is to run two separate regressions, one for rural and one for urban cells, with a cutoff of, for example, 10,000 inhabitants per cell. Results all variables remain robust to this sensitivity test until a cutoff of about 100,000 people per cell is used. This cutoff significantly alters the results, but the following results have to be interpreted with caution because only five cells had a population of 100,000 or above and remain in the sample. These new results are shown in Model 3 and 4. Zero-inflated adjustment is obviously not necessary any more for model 4.

Table 7: Model 3: Panel Poisson, Cell Fixed Effects, 100,000+ Population

Variable	**Coefficient**	**(Std. Err.)**
Difference in Strength	-4.143***	(0.892)
Past Violence	0.018***	(0.003)
Population (log)	-4.001***	(0.404)
Unemployment	-2.401	(1.530)
N	65	
LL(model)	-159.95	
LL(null)	-339.58	

Table 8: Model 4: Negative Binomial, 100,000+ Population

Variable	**Coefficient**	**(Std. Err.)**
Dependent Variable: total no. of Events		
Automobiles per inhabitant (interpolated)	-19.501***	(7.062)
Difference in Strength	-3.145 *	(1.774)
Past Violence	0.037***	(0.006)
Population (log)	-0.670 ***	(0.248)
Unemployment	1.617	(3.523)
Intercept	13.883	(4.824)
ln(alpha)		
Intercept	-0.710	(0.276)
N	65	
LL(model)	-188.16	
LL(null)	-228.40	

The wealth index has now a negative and significant influence on violence (see Model

4). Within the group of densely populated cells, wealthier cells experience less violence. In effect, this stratified sampling strategy is a hint towards a possible problem concerning cell wealth, the *Ecological Fallacy*. Cell wealth is measured by province. One (rich) city and its poor surroundings may have very different endowments of wealth and violence, but the coarse cell wealth indicator evens out this wealth distribution. Unless I find a more fine-grained indicator of wealth, at least as a sample at the city-level, it is advisable to interpret the higher wealth-more looting causality cautiously.

The low sample size induces me to also interpret the other results with caution. The "difference in strength" and the "past violence" coefficients remain robust. Within large cities, however, lower population increased the likelihood of violence. In addition, variation in unemployment does not explain variation in the level of violence in the big cities.

Multicollinearity and Endogeneity are no major issues in this paper. All variables are uncorrelated with each other. They are arguably also fairly exogenous, except perhaps the unemployment rate. For example, six out of the seven components of the Strength Equality Index can be assumed to be exogenous. The only concern is for the RRSI, but its variance is very low compared with government revenue.

An *Omitted Variable Bias* may be present in the zero-inflated negative binomial models. Some unobserved location-specific factor might affect both past and present violence. Part of the explanatory effect of the "past violence" variable might be traced back to these unobserved factors. By design, however, the reported fixed effects models account for this bias.

Spatial dependence and *stationarity* are also concerns to care about. If I estimate a random-effects panel (GLS) regression model (not shown) otherwise identical to model 2, I find that 98% of between-cell variance can be explained by this model. This means that spatial dependence is not an issue, and it is not necessary to control for violence in neighboring cells, for instance. However, only 20.6% of within-cell variance can be accounted for. Thus, further fine-grained research should focus on explaining the time series of the violence (Figure 3) not the geography.

A last minor source of error lies in matching names of towns in which violence occurred with their geographic location. Due to the transliteration from Arabic to English in the namefile (NGA 2006) and from Arabic to phonetic French in the news reports, the names of the places where the events occurred did not always match exactly. I tried to match these places with similar spellings or, all else failing, I reported only the province.

5 Conclusion

What has been done? The course of violence in the Algerian conflict can be explained with a "greed and grievance" framework; and further empirical evidence that "wanton" violence is actually often rational (Kalyvas 1998) is provided.

In Algeria, the government was able to control violence when it had overcome its deep

financial crisis. However, it was able to overpower the rebels only after its budget had grown to ten times the budget's size at the beginning of the conflict. The impact of this exogenous increase in oil prices provided 25 times more incremental funding to the government by 2005 than the IMF adjustment loans, thus probably benefiting the government much more. That proves that, unless very large amounts of support are provided to governments at war, violence will continue.

Violence is a very persistent phenomenon locally, over time. Even deducing the potential effect of omitted variables in this paper, hatred and violence spurs a spiral of further hatred and violence, and this is observable at the village-level. Policy-makers should thus worry about conflict contagion, but not worry if results from peace-creating efforts are not immediate. This "inertia of violence" effect can only be assessed very roughly within the framework of this paper, but violence in the past year raises current year violence at the local (village) level somewhere between 2% (model 3) and 54% (according to model 2).

Within countries, an actor that wants to end a war should try to build up its relative strength where the other actor's support is high. Small increases in relative strength at a location reduce violence very significantly. Power projection is probably best done for the government side by improving of the infrastructure, a part of government strength in my model.

Unemployment reduction efforts are likely one factor in shortening the war. The Bouteflika administation is notably criticized by the International Financial Institutions (e.g. IMF 2007b) that high government spending on unproductive employment creation programs may exacerbate existing dutch disease problems. In fact, what the government is probably doing is to efficiently buy off potential rebels. Low-scale violence is largely under government control, yet still, the insurgents manage to launch successful assaults from time to time as of 2007. With higher unemployment, it is likely that violence would surge. That buying off rebels is not a strategy sustainable in the long term is a point the Algerian administration is probably aware of. It knows very well, since the late 1980s, that it would be put in an unpleasant situation if the hydrocarbon prices were to plunge again.

In addition, this paper can provide evidence in support of some theoretical (Buhaug 2007, for example) and empirical models of civil war dynamics within a given country. Maybe a yet understated point of my paper is that *geographic* variance in civil war dynamics ought to be much easier to explain with few factors such as wealth, population, and revenge, than variance over time.

The results of the Liberia study (Hegre, Ostby and Raleigh 2007) are robust and confirmed with a larger number of events. Higher cell wealth can be assumed to be positively correlated with violence intensity. Even if the ecological fallacy caveats apply, results are robust except for extremely densely populated areas.

Finally, future research could focus on refining several concepts pioneered and refined by this paper, for example empirical operationalizations of loss-of-strength indexes. The empirical complexities of telling apart civil war from criminal violence are also worth further thought. Can robust rules be generated here? ACLED can probably become a good institu-

tion for providing rigorous coding rules concerning war-related violence. Further, one could investigate factors that reduce the severity of an empirical regularity found in this paper, the "inertia of violence". In addition, researchers must keep in mind that, in explaining a civil war's pattern, the really difficult part is to explain the severity time-series, not the geographic variance of violence. For the latter, simple models including past violence intensity and population density only will suffice as long as the data is complete enough. In addition, the new database underlying this paper can be used for further research concerning locally measured variables of interest. The high explanatory power of past events for current events is partly driven by omitted variable bias, as mentioned, and new ideas about which local variables I omitted and how to collect them in retrospect are a highly welcome, albeit tedious in data collection and coding, procedure.

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